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Economic Instability Trends and Levels Across Household Surveys

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Abstract

Understanding trends in economic instability is vital if economic and social policies aimed at mitigating economic risk are to be effective. Despite the popular perception that economic instability has been rising—and a research literature that often support this perception—recent studies have cast doubt on the conventional wisdom. At the same time, previous research that has used disparate measures, datasets, and methodological choices provides little guidance to account for different findings across studies. The diversity of findings leaves unanswered the question of whether different datasets yield similar conclusions when analyzed the same way. This report addresses this problem by estimating comparable trends in economic instability across three of the most-used household surveys—the SIPP, CPS, and PSID. I find that comparing estimates depends crucially on how imputed income components are addressed. If persons whose recorded incomes reflect in large measure imputed data are excluded, the SIPP and PSID yield similar results and indicate that economic instability has not changed much over 40 years. The CPS tends to show increases in instability, even after excluding incomes dominated by imputation. The higher imputation rates in the CPS—and their variability over time—compared with the SIPP suggest that CPS trends and levels may be more prone to bias, but the evidence is inconclusive.

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In the second half of 2010, the nation found itself in the midst of its third straight "jobless recovery", following the deepest recession in at least 25 years, with a "double dip" recession a very real possibility. The tenor of popular perceptions of economic conditions since the early 1990s is best conveyed by the reigning buzzwords of the era, including not only *jobless recovery*, but *white-collar recession*, *downsizing*, *outsourcing*, *off-shoring*, and *structural unemployment*.

This growing sense of insecurity has been accompanied by increasing policy and academic interest in economic instability, including its extent and whether or not it has risen. A team of researchers—backed by a prominent private foundation—has even suggested that an "economic security index" be added to the existing set of economic indicators used to evaluate economic conditions and set policy. The rapid increase in research on economic instability, security, and volatility, however, has in some ways proceeded too hastily. Basic questions of measurement have yet to be answered, such as whether different indicators (and types of indicators) produce similar results, the extent to which estimates depend on various methodological choices made by researchers, and whether different datasets produce similar estimates of the same indicator. In addition, the complexity of the available datasets raises the question of whether past estimates of instability are even accurate.

This report addresses a number of these questions by comparing estimates of economic instability produced from the three surveys best suited to examining instability. In so doing, it also assesses the levels of and trends in economic instability, measured as the risk of an earnings or income drop. This research is the first to consider patterns of economic instability across the three surveys, and to the extent possible I apply the same methodological decisions to each dataset. After a brief discussion of the surveys, I review the existing research findings from them. I then discuss the relevant methodological considerations and present my results.

The Surveys

Instability and volatility are inherently dynamic concepts focused on change over a relatively short period. As such, operationalizing them requires panel data tracking the same individuals over time. There are a limited number of panel surveys that can produce nationally representative estimates, but three surveys fit the bill—the Current Population Survey, the Panel Study of Income Dynamics, and the Survey of Income and Program Participation. These surveys differ in ways that are useful for examining how accurately and consistently economic instability is measured.[1]

[1] Other research has used the National Longitudinal Surveys (Abowd and Card, 1989; Buchinsky and Hunt, 1999) or administrative earnings data from the Social Security Administration (Mazumder, 2001; Dahl, DeLeire, and Schwabish, 2007, 2008; Gottschalk and Moffitt, 2007; Nichols and Favreault, 2009; Kopczuk, Saez, and Song, 2009; Dahl and Schwabish, 2009; Sabelhaus and Song, 2009), income data from

The Census Bureau's Current Population Survey generally is not thought of as a panel survey, though sample housing units are tracked for over a year. Because the design of the survey is complicated and because data files do not include unchanging cross-wave identifiers, few researchers take advantage of the longitudinal features of the data. CPS sample members are interviewed for four months, then one year later are interviewed in the same four months again. Because the sample is continuously refreshed every month, in any given calendar month, half of CPS respondents were interviewed twelve months earlier and half will be interviewed twelve months hence.

Respondents participating in March are administered the Annual Social and Economic Supplement (ASEC, formerly the Annual Demographic Supplement), which includes an extensive battery of questions about income received in the previous calendar year. Therefore, half of CPS sample members interviewed in March have detailed income data for both the previous calendar year and the year before that.

Because the supplement is administered around the time that taxpayers are preparing their tax returns, the income reports may be more accurate than in surveys where interviews occur at other times of the year. In addition, the relatively short duration of the panel makes sample attrition less of a concern than in other surveys. Furthermore, the survey content and questions themselves have been fairly consistent over time, in many cases going back to the early 1960s.

However, there are significant shortcomings of the CPS for longitudinal research. As noted, there are no unchanging person or household identifiers to link records separated by twelve months (or even one month). The absence of identifiers means that linking records requires matching on a set of variables that can cause errors in two different directions. If the set of variables is too restrictive, then many records will not be linkable, and particularly in the presence of recording or response errors, the matching process will fail to link all participants who could be matched. However, if the set of variables used to match records is not restrictive enough, records that do not represent the same person or household may be incorrectly "matched" while other records will not be linkable to a *unique* match. Both of these matching issues could affect estimates of economic instability. If those who are most likely to make mistakes in reporting their incomes, for

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the Internal Revenue Service (Slemrod, 1992; Carroll, Joulfaian, and Rider, 2007) or earnings data from unemployment insurance systems (Gottschalk, McEntarfer, and Moffitt, 2008; Celik, Juhn, McCue, and Thompson, 2009; Juhn and McCue, 2010). The NLS samples are based on birth cohorts followed over time, and so they are only representative of certain age groups. In addition, trends cannot be distinguished from life course changes because of this sample design (although estimates from different NLS panels can provide some trend information). Administrative data has great advantages over survey data, but are relative inaccessible, since privacy restrictions require analysts to apply to use the data (and often to analyze it only at certain sites). When linked to survey data, as in many of these studies, biased estimates may result if some survey members cannot be matched to administrative data, with refusal to provide a Social Security number a potentially large selectivity problem. Earnings data miss government workers who are not covered by Social Security, immigrants and others with no Social Security number, and workers paid under the table. Income data is available for "tax units" rather than families or households. Earnings are available in the Longitudinal Employer-Household Dynamics data, but only for a limited number of states.

instance, experience more instability, their absence from the matched sample will understate instability. On the other hand, incorrectly matching records will result in instability estimates that are likely to be too high.

These problems are exacerbated by a key feature of the CPS survey design—the survey samples *housing units*, not households. If a household moves away from its address, the CPS continues to focus on the address, interviewing the new occupants who replace the household. Therefore, households who are more residentially unstable will be less represented in the CPS than other households, making attrition bias a concern. In addition, if the linking of records is not conducted carefully, households who move may be "matched" to the new residents of their housing unit. While these issues clearly have the potential to affect estimated levels of economic instability, they will only affect *trends* in instability to the extent that their impact varies over time. Even in terms of levels, it is unclear whether CPS estimates of instability are likely to be biased upward or downward.

Another potential issue with the CPS is the fact that changes in household composition make income reports from the previous year difficult to interpret. Respondents are asked about the income received the previous year by current members of the household. But some of those household members may not have been present in the previous calendar year, while other people may have been present but are no longer members of the household. This issue is likely to increase measured economic instability.

Other concerns with the CPS include recall errors, since respondents must think back as many as 14 months about the income they received, and under-representation of disadvantaged groups due to nonresponse. As I will show below, survey and item nonresponse are more general problems that have gotten worse over time. Finally, because the CPS is generally used as a cross-sectional survey, there are no weights to address attrition.

The Panel Study of Income Dynamics, administered by the University of Michigan's Institute for Social Research, began in 1968 with a sample representative of the lower 48 states and an additional sample of disadvantaged households. It has followed these sample members ever since, interviewing them every year through 1997 and then every other year since then. What is more, the PSID has tracked sample members as they moved out of their original households, including young adults who leave to form independent households. The result is a rich multi-generational dataset. As with the CPS, respondents are asked about income received in the previous calendar year, and as with the CPS, questions are asked about a large number of income categories individually.

As a panel dataset following individuals, the PSID facilitates longitudinal analyses by providing person identifiers, avoiding the problems of matching presented by the CPS and its focus on housing units. Furthermore, because PSID surveyors have information on family members in other households for many potential attriters, the survey can rely on them to more easily track movers or other would-be attriters, an advantage not enjoyed by panel surveys that do not follow the branches of family trees across

generations. The PSID includes weights that adjust for attrition. In addition, perhaps because of the smaller size of the PSID relative to the CPS and SIPP or because of the ongoing participation of respondents, surveyors are able to elicit more complete responses from them, meaning that imputation of incomes is less of an issue. Remarkably, like the CPS, the survey covers a forty-year period.

The PSID shares some of the weaknesses of the CPS, including the lack of correspondence between current household composition and that from the previous year. In addition, recall error is likely a bigger problem in the PSID, since interviewing generally occurs later in the year.

Perhaps most important are issues that follow from the longitudinal, multi-generational design of the PSID. Sample attrition has been significant over the years, raising concerns that more recent estimates of economic instability will be more biased than in earlier years (potentially downward, if less stable households are more likely to fall out of the survey). Nevertheless, there is little evidence that attrition bias has been significant, although few studies have been conducted.[2] Another issue derives from the problem of following only original sample members and their offspring, which leaves immigrants and their families underrepresented (and increasingly so over time). To address this problem, the PSID added a refresher sample of post-1968 immigrants in 1997 and 1999. But this created a new inconsistency in that sampling weights were altered to make respondents representative of the national population whereas they were not meant to be prior to 1997.

The disadvantaged oversample in the PSID has been criticized for ambiguity in the methods used to actually sample respondents, with the sampling almost certainly nonrandom.[3] But if these respondents are excluded, there are no weights that adjust the main sample for attrition.

Finally, the administration of the PSID has changed more frequently and more dramatically than for other surveys. Questions used to collect income data (e.g., adding questions, combining or splitting income categories, shifting to asking about month-by-month receipt of selected income sources), top- and bottom-coding, income processing software, efforts to bring attriters back into the sample, rules about who to follow when households become disrupted, the length of the survey—all of these changed over time in potentially consequential ways. Importantly, many changes occurred between 1992 and 1996, raising concerns that there may be a seam during this period making later estimates incomparable to earlier ones.[4]

[2] Fitzgerald, Gottschalk, and Moffitt (1998a); Fitzgerald, Gottschalk, and Moffitt (1998b); Lillard and Panis (1998); Zabel (1998); Becketti et al. (1988). See Nichols and Zimmerman (2008), however, for evidence that from year to year, attriters are different from those included in volatility samples in terms of the joint distribution of a number of demographic variables.
[3] Brown (1996).
[4] See Winship (2009) for a review and discussion.

The final survey I use is the Survey of Income and Program Participation, also administered by the Census Bureau. The SIPP is actually a series of panel surveys, generally covering 32 to 48 months each. Like the PSID, it follows individuals as the unit of analysis, so when households or individual members move, they are pursued rather than just continuing to sample the occupants of the housing unit. As with the PSID, new household members are incorporated into the survey, though the focus is on original sample members. Participants are interviewed every four months and asked about the previous four months. SIPP panels have been followed since 1983, but with gaps between some panels.

The SIPP has a number of attractive features for measuring economic instability. Since respondents are asked about their incomes every four months, the survey likely has less recall bias than the PSID (although in theory the CPS may obtain more accurate calendar-year information due to the interview's proximity to the tax filing deadline). The short reference period also reduces the problem of household composition change. The SIPP's income questions solicit responses regarding more-detailed income categories than the CPS or PSID. Because the panels last only a few years, attrition is less of a concern than in the PSID, and because movers are followed, the problems of housing unit change in the CPS are avoided. Finally, the focus on monthly incomes means that sub-annual incomes may be examined.

The weaknesses of the SIPP include the fact that estimates are only available beginning in 1984-85 and that there are years for which estimates are unavailable. Panels are also short compared with the PSID. Imputation procedures changed in 1996, and imputation for at least minor income categories is fairly pervasive, growing more so over time. Variable names changed across panels more than once, and the coding of not-in-universe values changed. Finally, SIPP documentation and data is somewhat difficult to track down and navigate, and matching individual records across waves within a panel requires working with many files.

Previous Research

Research using the CPS generally shows wage instability among men rising over the 1970s through the double-dip recession of the early 1980s, then falling to late-1970s levels by the mid-2000s (Gittleman and Joyce, 1995, 1996; Cameron and Tracy, 1998; Celik et al., 2009). When self-employment earnings are also considered, male earnings instability shows a similar trend, except that instability does not fall after the early 1980s (Ziliak et al., 2010). Female earnings instability apparently declined dramatically (Ziliak et al., 2010). On the other hand, family and household income appear to have increased steadily through at least the late 1990s (Hertz, 2006, 2007; Ziliak et al., 2010).

Only two studies examine earnings instability using SIPP data. Peter Gosselin and Seth Zimmerman (2008) measure earnings instability as the average within-person variance over a three-year window. They find that instability was basically flat from 1984-85 to 2001-02. Sule Celik and colleagues (2009) examine the dispersion of the change in wage and salary income, confining the analyses to men. Consistent with the CPS research,

they find that instability declined from 1984-85 to 2006-07, fluctuating much more than in Gosselin and Zimmerman.[5]

These results are inconsistent with most of the SIPP studies that look at family or household income instability.[6] Family income instability apparently increased from the early 1990s to the early 2000s (Gosselin and Zimmerman, 2008; Acs, Loprest, and Nichols, 2009; Bania and Leete, 2007). Research that carefully treats imputation of income data in the SIPP, however, raises doubt about these findings. When Dahl, DeLeire, and Mok (2010) considered the risk of a 25 percent drop in household income, they found a modest increase from the early 1990s to the mid-2000s, but when they omitted individuals with imputed income data, the association between income instability and food insecurity increased and instability declined modestly over time.

Similarly, Dahl, DeLeire, and Schwabish (2008) found that the likelihood of a 25 percent change in household income (up or down) grew slightly from 1984-85 to 2004-05 but was flat once incomes with imputed labor income were dropped. They also showed that the trend and levels of household income instability when SIPP records are matched to Social Security Administration labor income data resemble the trend and levels when non-imputed SIPP data alone is used.Orzag (2008) presented matched SIPP-SSA results showing that the risk of a 50 percent drop in income was flat from 1984-85 to 2001-02 and that dispersion in income changes had declined.

The latest set of SIPP estimates measuring economic instability, from Jacob Hacker and his colleagues (2010a, 2010b), also struggles with how to deal with imputed income components. They exclude observations in any year that have "hot-deck" imputed data in any month of the year for head or spouse labor income. However, they retain observations with imputations based on cross-wave allocations, as well as those with imputation for other income components and other household members. They find that the risk of a 25 percent drop in household income rose unevenly from the mid-1980s to the early 2000s and then fell back to mid-1980s levels. They project on the basis of a regression model that instability jumped dramatically in 2008 and 2009.[7]

Dozens of studies on economic instability have been conducted using the PSID. In Winship (2009), I review about 20 PSID-based studies examining earnings instability and about 15 looking at income instability.[8] Based on these studies, earnings instability

[5] A number of studies use SIPP panels matched to Social Security Administration earnings records in order to take advantage of the demographic and contextual data in the SIPP while having the benefit of presumably more accurate administrative earnings data (Mazumder, 2001; Nichols and Favreault, 2009; Sabelhaus and Song, 2009). Since these studies do not use the earnings data in the SIPP, I omit them from the above discussion.

[6] Again, Nichols and Favreault (2009) match SIPP panels to SSA data to look at combined husband-wife earnings instability, but they do not use any of the SIPP income data.

[7] The authors focus on an "economic security index" that incorporates additional information on medical costs, debt, and wealth, but these additions mainly increase the level of instability without affecting the trend.

[8] Among the most important of these are Gottschalk and Moffitt (1994), Moffitt and Gottschalk (1995, 2008), Haider (2001), Dynan et al. (2008), Dynarski and Gruber (1997), Gosselin (2008), Hacker (2008), Rose and Winship (2009), Shin and Solon (2009), Stevens (2001), and Duncan et al. (1993). Since my

appears to have increased among men during the 1970s but to have declined slightly among women. There is little evidence for a secular increase in earnings instability since the early 1980s by most measures, and women appear to have experienced continual declines in instability. The exception is that instability as measured by within-person dispersion of earnings or by dispersion of shocks to earnings appears to have increased between 1980 and the early 2000s, at least according to the PSID.

Family and household income instability, however, has been consistently found to increase over the 1970s, 1980s, and 1990s in PSID-based studies. At the same time, in my recently completed doctoral thesis (Winship, 2009), I showed that when various methodological issues are addressed appropriately, the PSID indicates little secular increase in earnings or income volatility since the early 1980s recessions.[9]

Overall, most studies using the CPS, SIPP, and PSID paint a consistent picture in the aggregate. Earnings instability rose in the 1970s among men but probably did not rise much thereafter. It declined among women. Family and household income instability rose steadily from 1970 to 2000. However, as the issue of SIPP imputations suggests—and as my own work considering methodological decisions using the PSID affirms—this apparent consensus may be more fragile than it appears.

Methods

I use the PSID data available on the University of Michigan Institute for Social Research website and CPS data from Unicon Research Corporation. I also use the just-released 2010 CPS data from the Census Bureau's website. My SIPP data comes from the National Bureau of Economic Research website (1984-88 and 1990-93 panels) and the Census Bureau's SIPP website (1996, 2001, 2004, and 2008 panels). I use the SIPP core wave files in all panels rather than the longitudinal full panel files available from 1984-93, a total of 108 files in all. I also estimated results using the 1984 full panel file, which includes additional edits and imputations beyond those in the core wave files, to check whether the secular trends were affected. The 1984 estimates were very close regardless of whether the core wave files or full panel file was used, so I omit the latter.

For the PSID estimates, I use both the nationally-representative "SRC" sample and the disadvantaged "SEO" oversample, as well as the immigrant refresher samples added in 1997 and 1999. Doing so has both benefits and costs. On the benefit side, it maximizes sample size, and weights are available that adjust for attrition. The weights also adjust for other changes in the PSID sample from year to year. For instance, in 1990, 1992, 1993, and 1994, surveyors made concerted recontact efforts that resulted in the successful re-incorporation of several thousand attriters back into the survey. In 1990, 1993, 1994, 1996, 1997, and 2005, the PSID's rules for following household members that move were adjusted, and in 1994 the sample was redefined to include additional children. The SEO

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review, new PSID-based studies have emerged, including Gottschalk and Moffitt (2009), Bayaz, Chen, and Couch (2009), Leigh (2009), and three papers by Jonathan Heathcote, Gianluca Violante, and their colleagues (2009). See also Dynan (2010).

[9] See also Rose and Winship (2009).

sample was also reduced for budgetary reasons in 1997. These changes produce "seams" in the data that make year-to-year comparisons potentially problematic.[10]

Using the entire core sample also has costs. The initial selection of the SEO sample involved significant departures from random probability sampling.[11] Even absent this problem, using weights introduces inconsistencies into the data. With the incorporation of the immigrant sample, the weights use post-stratification to make the PSID sample nationally representative. Prior to 1997, the sample in any year is supposed to be representative of noninstitutionalized Americans alive in 1968, plus their descendants. From 1997 onward, the weighted PSID sample is supposed to be representative of the noninstitutionalized U.S. population for a given year.[12]

Fortunately, the decision as to whether the full weighted sample or the unweighted SRC sample should be used turns out not to be important—the estimates are very close regardless of which approach is chosen.

To match records in adjacent years of the CPS Annual Social and Economic Supplement, I first dropped the small number of cases in each year that were not uniquely identified by the matching variables. I then matched records for each pair of adjacent years that allowed for it, dropping non-unique matches and non-matches. The appendix provides details on how I conducted the matching, but essentially I required matched observations to have the same address and household identifiers across years, the same line number and sex, and age and education levels that were consistent.

Consistent Samples

Because policy debates over economic instability have focused on changes in families and in labor markets, I focus on working-age adults. Specifically, I restrict the samples to adults between the ages of 20 and 59 during periods over which income changes are examined (generally either one- or two-year intervals). In the CPS and PSID, I subtract one from individuals' ages at the time they are interviewed (since income questions refer to the previous year). In the SIPP I measure age as of each month referenced by income questions. The age range I use automatically excludes most students and retirees, but in most analyses I conducted sensitivity checks in which I explicitly excluded them.[13] The results I present are not sensitive to their inclusion.

The two other major sample restrictions I make are to exclude occupants of group quarters and those with non-positive incomes. The latter exclusion is a key source of differences across existing research, but CPS data demonstrates that those without income are generally special cases whose circumstances are not centrally about labor markets. In 2008, the primary reason that men went the whole year without working was a sickness or disability, and when students, retirees, and homemakers are added, 81

[10] Gouskova et al. (2007). Heeringa and Connor (1999).
[11] Brown (1996).
[12] Heeringa and Connor (1999).
[13] As discussed below, excluding adults with no income also screens out retirees and students.

percent of male non-workers are accounted for. Just 13 percent said that they could not find a job. The figures in 1967 were 88 percent and 5 percent. Among women, half of those who did not work in 2008 were homemakers, and only 4 percent could not find work, versus 91 percent and 1 percent in 1967. Families and households with no or negative income generally include someone with a business who reports a loss for the year, but such circumstances are uncommon.

After dropping non-positive incomes, I trim the top and bottom 2 percent of remaining incomes within a small number of age categories.[14] This approach mitigates the problem of top-coding and changes in top-coding over time. It does *not* address top-coding of the various *components* of aggregate income figures. To do so, I checked the sensitivity of my results using CPS data generously provided by Richard Burkhauser. Burkhauser and his colleagues created annually consistent topcodes for the income component variables using internal restricted-use data at Census Bureau facilities (Larrimore et al., 2008). They then built back up the aggregate income variables. My results did not change appreciably by using these variables (results available upon request).[15]

Finally, in some years in the SIPP and CPS I checked the robustness of my results by excluding not only those retired or in school, but those who at any time were out of the labor force because they were sick or disabled, or taking care of someone at home; those who were in the armed forces; those who experienced a birth in the previous year; and those who were self-employed. In general, these restrictions had very little effect on trends and surprisingly little effect on levels.[16]

Imputations

A particularly important set of sensitivity checks is to see how the results change depending on how imputed data is treated. The CPS has two types of imputations—item imputes and whole imputes. The latter involves imputing values for all items for households that sit through the basic monthly CPS interview but do not participate in the ASEC supplement.[17] The incidence of whole imputation has been fairly constant over time, but the number of item imputes increased steadily between the 1992 and 2003 surveys, with the overall percent of income imputed rising from just over 20 percent to nearly 35 percent.[18] Imputation rates fell off somewhat thereafter but remained above 30 percent.

Basic monthly CPS non-interviews do not have imputed values in the ASEC supplement; the ASEC weights incorporate adjustments to account for demographic differences in who is successfully interviewed. The rate of "type A" non-interviews (non-interviews

[14] The categories are ages 20-29, 30-39, 40-49, and 50-59. For family and household income, for the CPS and PSID, the trimming is done only among heads, and other family or household members are then assigned this income (or trimmed out if the head is).
[15] I also estimated results when using a 1% or 4% trim in the CPS, which affected levels but not trends. Using a 1% trim in the PSID, however, does sometimes change trends, which is why I use a 2% trim.
[16] I exclude non-civilian adults from all CPS analyses as well.
[17] I thank Charles Nelson for helping me identify whole imputes (one uses the variable FL-665).
[18] Turek et al., 2009; U.S. Census Bureau, 2006

that are eligible for the CPS) was stable from 1964 to 1993 but rose from around 4.7 percent to 6.9 percent between 1993 and 1995 and remained around 7 percent thereafter (U.S. Census Bureau, 2006). The change coincided with the shift to computer-assisted interviewing. Combining the non-response with imputation, the share of households eligible for interviews that have non-imputed income data fell from around 75 percent in 1991 to around 60 percent in 2003. The higher non-response and imputation rates in recent CPS surveys translate into an even lower share of my sample with non-imputed data in both of the two matched years. As I will show below, the fraction of cases in my sample with household income that includes imputations in one of the two years soared to nearly 80 percent by 2003 and then fell precipitously.

Imputation is even more common in the SIPP, where roughly 80 percent of cases had at least one component of household income in at least one month estimated through imputation. Imputation procedures also changed notably in the 1996 panel. For both the CPS and the SIPP estimates, I determined what percentage of each person's income was imputed. I then produced estimates excluding adults with any imputations and excluding adults with 75 percent of their income or more imputed.[19] I also ran several additional sets of CPS results, excluding cases with whole imputes in either year or with relevant item imputes in either year. For the PSID, where imputation rates are much lower, I simply produced results with and without cases that have imputations.

Weights

I use sample weights in all analyses. I select the weight for the last observation in the span of time being considered. That means calendar year weights for the CPS and PSID. For the SIPP estimates, I weight by calendar year weights for the 1996, 2001, and 2008 panels, but only cross-sectional weights are available for other panels. In those cases, I weight by the last month in the span of time being considered. I also confirmed that the 1996-2008 estimates were similar if I used the last month's cross-sectional weight instead of the calendar year weight. The results are relatively insensitive to the choice of weight used. For the "quadrimestral" income analyses described below, I use the last-month cross-sectional weights for all panels.

Measuring Economic Instability

Broadly speaking, there are two ways of thinking about economic instability. On the one hand, the risk of a large drop in income may engender a "fear of falling" among many families. This form of instability is primarily concerned with sudden changes in living standards that make it difficult or impossible to maintain one's preferred lifestyle.

[19] The CPS calculations of the percent of income imputed account for whole imputes only from 1988 forward, as I could not identify such cases before then. My calculations for the SIPP take into account not only allocation flags for individual income components but "type Z", "pseudo-type z", and "little type z" non-respondents as well as "EPPFLAG imputations" (U.S. Census Bureau, 2001). Little type z and EPPFLAG imputations, which apply to adults for whom there is no information available about whether they worked during the reference period, are not necessarily reflected in item allocation flags. I also tried excluding SIPP adults whose data was provided by a proxy member of their household, but this had little effect on trends or levels.

On the other hand, *volatility* in income—unpredictability due to the magnitude or frequency of swings up and down—may increase insecurity by the threat of an income drop but also simply by making it difficult to plan and make important decisions related to work, savings, family formation, childrearing, education, and retirement. For a given long-term income level, it matters whether the path of income is free of sharp fluctuations or whether income bounces around wildly from year to year, ultimately netting out.

I focus on the first conceptualization of instability here, examining the probability of experiencing a 25 percent drop in income over a one- or two-year period. This conceptualization, I would argue, is what is mainly of social and political concern. Indeed volatility, conditional on the risk of an income drop, is a good thing, as it means that recovery from drops is common. Furthermore, the question of whether to add an economic security index to the national economic indicators currently centers around a measure similar to mine.

I save for future work a look at the second conceptualization economic instability. Most of the measures that have been used to date require more than two periods of income data for each person, meaning that the CPS cannot be used to produce such measures, and only limited measures are possible using the SIPP (and only starting in the mid-1990s). Interpreting two-period measures of instability—such as those examining the dispersion of income changes, or autocorrelations—is tricky. For instance, greater dispersion of income changes may constitutes greater instability, since it implies that relatively large income changes are becoming more common and relatively small income changes less common. However, it is also true that the dispersion of income changes can increase simply because living standards are steadily rising across the board or because inequality is increasing.[20]

The CPS ASEC Supplement and the PSID include only annual income measures. Only one-year intervals between two interviews are available in the CPS ASEC. The PSID has one-year intervals through 1996 but only two-year intervals thereafter. SIPP panels can be used to look at one-year intervals or, in some panels, two- or three-year intervals. They also include monthly income measures. I aggregate monthly incomes up to calendar year totals. However, newly released data from the 2008 panel of the SIPP allows four-month income totals to be compared in 2008 and 2009. In some analyses, I show trend estimates based on this "quadrimestral" income measure, reflecting income received from August through November of each year (August 2008 being the first month for which income data is available in the 2008 panel, and November 2009 being the most recent month). These provide a benchmark against which to evaluate Hacker et al.'s projections for 2008 and 2009.

[20] Winship (2009) proposes a measure of "pivot volatility" that examines the frequency and magnitude of individuals' income reversals within a nine-year window. The measure shows a much smaller increase in instability over time than measures of the dispersion of income changes.

Income Measures

I focus on four different income measures below: the wage and salary income of male heads, the earnings of men, women's earnings, and pre-tax post-transfer household income. Wage and salary income is available in the PSID only for "family unit" heads, who by default are men whenever a couple heads the family.[21] In the CPS and PSID, I focus on household heads but confirmed that trends and levels for family heads were similar.

Earnings includes not only wages and salaries, but self-employment income. In the CPS, I focus on the earnings of household heads and spouses, while in the PSID I include long-term cohabiting partners of family unit heads as well. In the SIPP I include earnings of only male household heads or, among women, heads, wives, and cohabiting partners.[22]

I also conducted sensitivity analyses examining family income and household income adjusted for the number of people in the unit. I also used the Cross-National Equivalent File (a version of the PSID that includes tax estimates generated from NBER's TAXSIM application) to examine post-tax-and-transfer income. In general, pre-tax income, size-adjusted pre-tax income, and post-tax income show similar levels and trends, both for household income and for family income (with family income estimates slightly higher, results available on request).

Finally, using the CPS, I constructed more comprehensive measures of income, adding the value of employer-provided health insurance to wage and salary and labor income and also adding to household income net taxes, net capital gains, the value of Medicaid and Medicare, the value of school meals and food stamps, the value of subsidized housing, and the value of the flow of services from home ownership. In general, the results were largely unaffected.

All incomes are adjusted to constant 2009 dollars. In the PSID and CPS, I use annual PCE deflators, while in the SIPP I use monthly CPI-U-RS adjustors (because monthly PCE deflators are not available in recent years).

Results

Male Head Wage and Salary Income; Male Earnings

Figure 1a compares trends for the probability of a large (25 percent) one-year drop in wage and salary income among male heads. In all charts, I label pairs of years according to the second (later) year. Lines in the chart are linear interpolations between data points.

[21] In the PSID a family unit is defined as "a group of people living together as a family. They are generally related by blood, marriage, or adoption, but unrelated persons can be part of a FU if they are permanently living together and share both income and expenses." (http://psidonline.isr.umich.edu/Guide/FAQ.aspx#90) There are a small number of households with more than one family unit.

[22] SIPP treatment of business income changed in the 2004 panel, but when I re-ran results different ways to address the change, there was little effect on the results (available from the author on request).

Over the 1970s and the first half of the 1980s, the CPS and PSID trends align fairly well, with the CPS estimates consistently higher than those of the PSID. However, the CPS shows increasing instability while the PSID trend over this period is more ambiguous. Beginning in the mid-1980s, the CPS trend clearly departs from the PSID and SIPP estimates and rises steadily. From 1970 to 1996, the risk of a drop in wage and salary income actually falls from 10 percent to 9 percent in the PSID, but in the CPS it rises from 10 percent to 16 percent. From 1986 to 2007, instability in the CPS increases from 13 percent to 19 percent, but the SIPP shows a decline from 12 percent to 9 percent. Notably, the trends and levels for the PSID and SIPP align very closely for the years where they overlap (1986-1995).

If the PSID and SIPP trends are considered as a single series, the implication is that wage and salary instability has largely followed a cyclical pattern, rising with contractions in the economy and falling during expansions. Instability estimates for 1970 and 2007 are basically the same.

This conclusion challenges the common perception that the U.S. economy has undergone a "great risk shift" in recent years. In addition, the SIPP estimates for instability in quadrimestral, or August-to-November, wage and salary income imply that the recession did not raise instability levels to unprecedented heights. Comparing late 2009 to late 2008 reveals a similar level of instability as in the recessions of the early 1980s and early 1990s, though of course instability may turn out to be higher still when the 2010 estimates become available.

On the other hand, in the CPS, the risk of an income drop more than doubles over forty years. What accounts for this stark difference? Figure 1b suggests a possible answer, showing rates of imputation for each survey for each pair of years over which an income drop is measured. The high imputation rates in the SIPP and CPS stand out against the low rates in the PSID. Furthermore, from 1988 to 2002, imputation increased dramatically in both the CPS and SIPP, so that roughly 40 percent of heads in my early 2000s samples had wage and salary data that was wholly or partly imputed in one or both of the years over which drops are measured.

There are, however, three potentially important differences between the SIPP and CPS when it comes to imputations. First, in the SIPP, imputation became much less common after 2002, while it declined more slowly in the CPS. Second, while nearly all cases in the CPS with imputations consist predominantly of imputed data, the share of male heads with at least 75 percent of their wages and salary imputed is low in the SIPP—around 10 percent. Third, this share is stable over the SIPP panels and waves, while it rises steadily in the CPS through the early 2000s. All in all, these patterns point to a greater likelihood that whatever bias is present in the estimates in Figure 1a, the SIPP trend is less likely to be biased than the trend for the CPS.[23]

[23] Computing the fraction of cases with imputed incomes prior to the 1976 survey is not possible because allocation flags refer to a person's entire family rather than to individual income. The choice of a 75

Figure 1c presents the same trends as Figure 1a after excluding men with wage and salary estimates dominated by imputations, and it goes a long way toward reconciling the three surveys' results. When looking only at male heads with non-imputed (or, in the SIPP, mostly non-imputed data), the trends in instability across the three surveys is completely consistent through the early 2000s. Instability hovered around 10 percent throughout the 1980s and 1990s.

From 2002 onward, the CPS trend still departs from the trends in SIPP annual and quadrimestral income. Because the CPS trend does not fall from 2002 to 2007, the rise in instability common to both datasets after 2007 results in higher instability in the CPS—a 16 percent risk of a large drop in wage and salary income rather than the 12 percent risk shown in the SIPP. The fall in instability shown in the SIPP is consistent with the cyclical pattern in previous years, however the absence of this decline in the CPS is consistent with the idea of a jobless recovery over the aughts.

The annual PSID estimates extend only through 1996, but biennial estimates are available thereafter. Figure 1d shows that the PSID and SIPP figures for the risk of a two-year drop of 25 percent or more are fairly consistent, especially when imputations are accounted for. The estimates are somewhat difficult to compare, however, since there are only three years in common (1995, 1998, and 2006). Through 2004 the estimates track each other very well. It is highly likely that the decline in the SIPP from 2003 to 2006 is steeper than what the PSID would show. Figure 1d also brings over some of the CPS estimates from Figure 1c for comparison (two-year drops cannot be computed in the CPS, since no one remains in the sample for that long). The 2006 PSID estimate is roughly equidistant between the CPS and SIPP estimates, though the CPS remains the only one of the three to show an increase in instability from 2002 to 2007.

Figures 2a through 2d provide analogous results when expanding the sample to men who are spouses or cohabiting partners of the head and expanding the earnings measure to incorporate self-employment income. The figures mirror those for male heads' wage and salary income, with instability levels slightly higher but trends no different. The only exception is that in Figure 2d, the one-year CPS estimates for the mid-2000s are similar to those in the PSID and SIPP, though the trends over the decade still differ markedly.

Female Earnings

Trends in and levels of economic instability across the three surveys are even more consistent for women than for men, once imputations are accounted for. In Figure 3a, the PSID and SIPP results again line up very well, showing a secular decline in instability. The CPS estimates again diverge, showing a small increase over time.[24] Figure 3c, however, shows that when women with imputed earnings are excluded, the three surveys

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percent threshold is arbitrary, but excludes only cases that are clearly dominated by imputed data. Among cases excluded based on this criterion, the vast majority have their entire earnings imputed.

[24] Interestingly, the increase is larger when the value of employer-provided health insurance is added to earnings. Results available upon request.

show very similar levels, and all show declines in instability, flattening out in the last 15 years. The flattening out is more apparent in the two-year-drop estimates in Figure 3d.

Household Income

Finally we come to household income instability. Figure 4a shows the familiar rise in instability in the CPS, but this time, the PSID also shows a steady rise. In contrast, the two sets of SIPP estimates show little secular change.

Figure 4b displays imputation rates. The SIPP and CPS have remarkable shares of cases with at least some imputed components. In the SIPP, it approaches 90 percent and nearly reaches 80 percent in the CPS. However, when focusing on cases where 75 percent of income is imputed, the rates are far lower, ranging from 10 to 20 percent in the SIPP and 20 to 40 percent in the CPS. Imputation nevertheless rises steadily in the CPS before leveling off. The stability in SIPP imputation rates (and their relative infrequency) again suggests that the SIPP trend in instability is less likely to be affected by imputation compared with the CPS trend.

Excluding cases dominated by imputation brings the CPS and PSID trends closer to the SIPP ones, as shown in Figure 4c, but the former still show an increase over time. If the CPS and PSID are taken as a single time series, then income instability doubled from the late 1960s to the late 2000s. In contrast, the SIPP estimates reflect a clear cyclical pattern and no secular increase. The quadrimestral estimates show that income instability was no higher from 2008 to 2009 than from 1990 to 1991.

Finally, Figure 4d—showing trends in instability measured over two years rather than one—indicates that the increase in the PSID is actually smaller than Figures 4a and 4c might suggest. The absence of one-year-drop estimates after 1996 in the PSID obscures the fact that instability falls notably in the PSID during the 1990s recovery. This is consistent with the SIPP estimates, but not the CPS ones. Instability still rises in the PSID—even when excluding cases with imputations—by over five percentage points, but the late-1990s trough is the lowest since 1973, and the early 2000s peak is barely higher than in the mid-1970s and early 1980s. In the SIPP, instability is the same in 2006-07 as in 1993-94.

Conclusion

My review of work to date on economic instability characterized the research as concluding that male earnings instability rose in the 1970s and has remained at those higher levels thereafter and that family and household income instability has risen steadily since the early 1970s. Of the research that specifically looks at the risk of a large short-term earnings or income drop, most studies find increases over time.[25] The exceptions to this conclusion have come from studies using the SIPP, often linked to

[25] Dynan, Elmendorf, and Sichel (2007, 2008); Gosselin (2008); Gosselin and Zimmerman (2008); Hacker and Jacobs (2008); Hacker et al. (2010a, 2010b); and Hertz (2007).

administrative data on earnings.[26] Researchers have not offered compelling reasons for why the SIPP results differ from the existing CPS and PSID results, but to the extent that they have acknowledged the lack of correspondence, they have not challenged it.

The analyses here suggest that when imputations are addressed adequately, the three major panel household surveys yield conclusions that are not that far apart, with the SIPP and PSID indicating that there has been little secular change in economic instability (except for a large decline in instability in women's earnings). The CPS is more of an outlier after 2000, for reasons that remain unclear.

These analyses cast doubt on the conclusions of an ongoing high-profile effort to track economic security using a measure based on the risk of a large income drop. Jacob Hacker and his colleagues report that economic insecurity and instability has been rising steadily since the late 1960s and is currently at a level never before seen over this period. In contrast, I find using the SIPP—contrary to their estimates using the same dataset—that there was no secular increase in income instability from the mid-1980s to the mid-2000s and that the recent increase associated with the recession put instability at a level no higher than the recession of the early 1990s. The PSID shows somewhat of an increase over time, but the increase is small—from a 10 percent risk of a 25 percent income drop over two years to a 16 percent risk. Much depends on what specific years one chooses to compare. The CPS shows a steady rise, but this occurs concurrently with a doubling of cases dominated by imputation.

Nor has male earnings instability risen in the SIPP or PSID in recent decades. The CPS shows an increase stemming from the failure of estimates to fall during the 2000s recovery, but this conclusion is subject to the very large caveat that well over a third of the men in the sample have all or nearly all of their earnings imputed in at least one of the two years during which they are observed, and the incidence of imputation increased markedly over time.

The consistency of the SIPP trends for male earnings and household income, for annual income comparisons and quadrimestral income comparisons, and whether cases dominated by imputation are included or omitted is striking. Furthermore, the SIPP's greater interviewing frequency and shorter recall—which also mitigates against the problem of household compositional change—would seem to be clear advantages over the CPS and PSID. The CPS has high and time-varying imputation and survey nonresponse rates, and algorithms for matching people across two March surveys are far from perfect. The PSID's attrition over time and administrative changes call the validity of estimates from the survey into question.

While the evidence retains some ambiguity, the similarity of the PSID and SIPP estimates after addressing imputations suggest that estimates from the two data sets can usefully be combined to produce time series extending from the late 1960s to 2009. Those time

[26] Acs, Loprest, and Nichols (2009); Dahl and Schwabish (2009); Dahl, Schwabish, and DeLeire (2008); Dahl, DeLeire, and Mok (2010). Acs et al., however, find an increase in the risk of a 75% income drop from one quadrimester to the next.

series generally refute the idea that economic instability has risen substantially. This conclusion could be solidified if future research is able to show that the departure of the CPS trends is due to survey differences and if it finds similar results using other measures of economic instability.

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Appendix: Matching March CPS Files

Matching individuals' data from one March CPS survey to the following year's is no easy task. Not only is there no complete resource available offering instruction to the data analyst, but various aspects of the data require special handling, and the result is an aggregate file with 1.5 million potential matches. The following discussion draws from Unicon Research Corporation (2006) and U.S. Census Bureau (2009).

To match records, one must first match on year (which, of course, increments by one) and month in sample (which increments by four because the second year's March interview is fourth interview since the sample address's first March interview). Then one matches on address and household identifiers. Unfortunately, the variables needed change over time, the address identifiers are not unique in some years, they are corrupted in a few files, household identifiers are not necessarily unique within addresses, and periodic sample redesigns mean that addresses cannot be matched across some pairs of years.

Finally, one matches individuals within households on their "line number" and other characteristics that uniquely identify them. Line numbers are unavailable in a few years. The more variables one uses to match individuals, the more likely it is that matches will be unique, but the more people will be improperly excluded due to response, transcription, or coding errors in one of their two interviews. The fewer variables one uses, the more matches one will get, but many of them will be non-unique, and some of the unique matches will be improper because they will link two different people who happen to have the same line number and characteristics (particularly problematic when the occupants of an address change).

For the 1968 to 2004 files, the matching variables I used included (with Unicon/CPS ASEC variable names): year (_year/H-YEAR), month in sample (mis/H-MIS), household id (_hhid/H-IDNUM or H-IDNUM1), household number (hhnum/H-HHNUM), line number (lineno/A-LINENO), sex (_male/A-SEX), age (age/A-AGE), and education (grdhi, grdcom, and grdatn/A-HGA and A-HGC). For the 2005 to 2010 files, I also matched on hhid2/H-IDNUM2 for 2005-10 (which includes hhnum/H-HHNUM within it and thus makes keeping that variable in the match redundant). As described below, matching 1973-74, 1975-76, and the files since 2004-05 required additional steps.[27]

In my choice of demographic variables, I confined the set to sex, age, and education as variables that could be expected to be reasonably consistent across interviews. Age could be as much as two years higher and education as much as one year higher in the second year matched (and no lower in either case). The education restriction from 1991 to 2010 uses the algorithm of Madrian and Lefgren (2000), since the variable is a categorical attainment indicator rather than representing years of schooling. The "looseness" of the

[27] Note that the "household" id is actually an identifier for the housing unit, or address. "Household number" is incremented every time a new household resides at the address, with one exception. If an address is deemed a "type b" or "type c" noninterview (essentially, if the unit has become unoccupied, demolished, or converted to nonresidential use), after at least one successful interview, the household number is not incremented once the unit becomes occupied again (Nekarda, 2009).

age and education restrictions means that I include some improper matches of different people with similar education and age levels. I decided against using race as a matching variable because of concern that reporting by multiracial and Hispanic adults might not be consistent, because the race variable in the CPS changed to allow multiple categories to be selected in 2003, and because evidence in Madrian and Lefgren (2000) suggests that it adds little to matching precision beyond sex, age, and education (see their Table 3).

I require unique matches on all three variables (in the notation of Madrian and Lefgren, I use merge criterion s|a|e). Among the different criteria they examined, the one that I use was one of the two best at excluding people who indicated they had lived at a different address on March 1 of the previous year and one of the two worst at including people who indicated they lived at the same address. However, all of the roughly three dozen criteria examined included 92 percent or more of non-movers, while just ten excluded 20 percent or more of movers. My merge criterion errs on the side of excluding many matches inappropriately in order to appropriately exclude more matches. Evidence presented by Madrian and Lefgren suggests that the vast majority of people I exclude using my criterion reported that they lived at the same address a year earlier.

Matching is not possible for five pairs of years. In 1972, the address identifier is incompatible with 1971 (because of changes in sampling procedures) and with 1973 (because the "random cluster" codes changed). Because of changed variable formats, 1976 cannot be matched to 1977. Matching of 1985 and 1986 is not possible due to a change in the sample design, and sample design changes prevent 1995 and 1996 from being matched.

Other pairs of years require special treatment. Many of the address identifiers from the original 1973-1975 Census tapes are corrupted, with occasional blanks in the middle of observations' values.[28] While the 1974-75 match was nevertheless as successful as in other years, very few cases matched across 1973-74 or 1975-76. Therefore, after the initial match attempt, I conducted a second round of matching to link additional records from 1973 to 1974 and from 1975 to 1976. This time I matched on the two non-corrupted parts of the identifier—segment number (segnum) and serial number (sernum) rather than _hhid.

The 1975-76 match is trickier still. There is no line number in 1976, so the match must omit this variable. In addition, the household identifiers for the 1975 records must be recoded—the third-to-last digit of _hhid must be cut.[29]

[28] To my knowledge, this is the first time this problem has been discovered. I confirmed that it exists in the Unicon data, the CPS files on NBER's website, and the files on ICPSR's website.

[29] When using the Unicon data, hhid must also be substituted for _hhid in the 1976 records to match 1975 to 1976 because Unicon codes _hhid as hhseq in 1976. Also, the Unicon data requires that, before conducting the secondary match, the segment number variable for 1975 be replaced by extracting a four-digit version of the segment number from _hhid. I do not know whether the same replacement is required when using the original CPS files. Between 1992 and 2010, the _hhid variable has leading zeroes in some years but not in others, so it must be coded consistently for matching. To match 1993 to 1994, the state variable must be appended to _hhid in 1993, since Unicon appends it to 1994 for _hhid to produce more

Line numbers are also missing from 1977 and 1978, so the match for this pair of years takes place without them.

Finally, due to the ASEC sample expansion in 2001 to improve estimates of low-income children without health insurance, many sample members from 2001 to 2010 are not actually interviewed in March and are not administered the ASEC in consecutive years. Their inclusion in the ASEC files creates additional non-matches and mismatches compared with earlier years. Feng (2008) notes, however, that these sample members may be excluded by matching the ASEC records to March basic monthly CPS records (those sample members who receive the basic CPS interview in March, prior to receiving the ASEC supplement). This match is straightforward for 2005-10, and I do so prior to matching ASEC supplements. I used basic monthly survey files extracted using the Census Bureau's DataFERRET software, matching to ASEC files on year, mis, _hhid, hhid2, and lineno. For 2001-04, matching the ASEC and basic monthly files appears to be impractical due to the absence of the hhid2 variable (which incorporates not only hhnum but the sample ID and serial suffix codes that are available as separate variables in the basic monthly file).

Table A1 provides matching rates and totals for each pair of years. My final sample of uniquely matched records—non-unique matches are dropped—includes nearly 890,000 adults between the ages of 21 and 60, or about 24,000 per year on average. Across years, I match an average of 62 percent of those adults who potentially could be matched. Changes in residence, attrition, and deaths make up part of the difference.

The accuracy of this and previous matching attempts could be improved by identifying errors in the March data using a process outlined by Feng (2004). The idea is to compare variable values in one month to the sample members' responses in other months. If, for instance, someone reported a different sex in the January, February, and April basic monthly surveys than in March, one would infer that the March value is an error and recode it. Whether this would *profitably* improve the accuracy of the matching results here, however, is questionable.

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unique address identifiers. Presumably, the user of the original CPS files should also match on state for 1993-94.

Table A1. Summary of Matching Process, ASEC Current Population Survey Files

				Matched on						
	Base Sample	Minus non-March BMS	Year 2 Sample	Address & HH ID's	Plus Line No.	Uniquely on Line No.	Plus Line No., Sex, Age, & Ed	Uniquely on All 4	Plus Secndry 1973, 1975	% Uniquely Matched[a]
1968	34,796		35,104	26,884	25,371	25,306	24,285	24,285		69.8
1969	35,668		33,681	26,114	24,736	24,705	23,609	23,609		70.1
1970	33,928		34,089	26,594	25,066	24,984	24,041	24,041		70.9
1971	34,776		0	0	0	0	0	0		0.0
1972	0		32,382	0	0	0	0	0		0.0
1973	32,438		32,583	16,437	15,497	15,438	14,815	14,815	21,897	67.5
1974	31,140		31,239	24,113	22,761	22,695	21,594	21,594		69.3
1975	31,488		32,822	16,644	16,644	1,989	14,897	14,758	21,896	69.5
1976	32,817		39,425	0	0	0	0	0		0.0
1977	39,155		38,919	28,317	28,317	3,552	25,215	24,989		64.2
1978	38,347		38,724	27,891	27,891	3,627	24,754	24,440		63.7
1979	38,867		45,829	28,630	26,745	26,592	25,032	25,032		64.4
1980	46,031		46,958	35,103	32,918	32,708	30,925	30,925		67.2
1981	45,784		41,663	31,165	29,067	28,861	27,383	27,379		65.7
1982	42,003		41,988	31,744	29,500	29,254	27,779	27,779		66.2
1983	42,292		42,112	31,334	29,060	28,785	27,233	27,231		64.7
1984	41,995		42,649	29,959	27,834	27,604	26,232	26,232		62.5
1985	42,031		41,378	0	0	0	0	0		0.0
1986	41,398		40,979	29,433	27,323	27,039	25,291	25,287		61.7

(continued)

Table A1. Summary of Matching Process, ASEC Current Population Survey Files, Continued

				Matched on						
	Base Sample	Minus non-March BMS	Year 2 Sample	Address & HH ID's	Plus Line No.	Uniquely on Line No.	Plus Line No., Sex, Age, & Ed	Uniquely on All 4	Plus Secndry 1973, 1975	% Uniquely Matched[a]
1987	41,170		41,268	30,055	28,009	27,829	24,540	24,540		59.6
1988	41,228		38,264	27,875	25,946	25,946	23,984	23,984		62.7
1989	38,291		41,728	28,376	26,466	26,466	24,336	24,336		63.6
1990	42,119		42,646	30,957	28,702	28,702	26,482	26,482		62.9
1991	41,580		41,936	30,378	28,311	28,311	24,220	24,220		58.2
1992	41,253		41,282	30,267	28,134	28,134	24,769	24,769		60.0
1993	41,458		43,210	29,656	27,626	27,618	23,866	23,866		57.6
1994	36,836		39,764	26,462	24,670	24,520	21,802	21,798		59.2
1995	40,303		34,415	0	0	0	0	0		0.0
1996	34,679		34,626	25,871	24,129	24,125	21,062	21,062		60.8
1997	35,497		35,085	26,310	24,503	24,503	21,452	21,452		61.1
1998	35,417		35,294	26,381	24,600	24,600	21,451	21,451		60.8
1999	35,718		39,673	26,341	24,585	24,580	21,520	21,520		60.2
2000	32,317		57,665	25,926	24,169	23,821	21,145	21,129		65.4
2001	57,558		58,143	32,916	30,385	28,021	25,089	25,041		43.5
2002	57,527		57,670	33,505	30,903	28,465	25,455	25,385		44.1
2003	57,869		57,539	33,721	31,163	28,672	25,687	25,602		44.5
2004	55,862		38,325	27,433	25,547	24,701	21,681	21,669		56.5
2005	55,186	36,203	36,227	28,189	26,351	26,351	22,827	22,827		63.1
2006	55,902	37,135	36,295	28,757	26,941	26,941	21,280	21,280		58.6
2007	55,419	36,554	36,435	28,980	27,172	27,172	22,817	22,817		62.6
2008	54,579	36,179	36,307	28,988	27,125	27,125	22,723	22,723		62.8
2009	55,280	36,668	36,718	29,456	27,502	27,502	23,230	23,230		63.4
Total[b]	1,578,075	182,739	1,505,439	1,047,162	981,669	907,244	874,503	873,579	43,793	61.0
Average[b]	42,651	36,548	40,688	28,302	26,532	24,520	23,635	23,610	21,897	61.9

Notes: See Appendix for details. [a]Percent matched is the number uniquely matched divided by the starting observations in the year with the smallest starting sample. [b]Total and Average are based on the 37 pairs of years retained (excluding 1971-72, 1972-73, 1976-77, 1985-86, and 1995-96).

Figure 1a. Percent of Male Heads Experiencing One-Year Declines in Wage and Salary Income

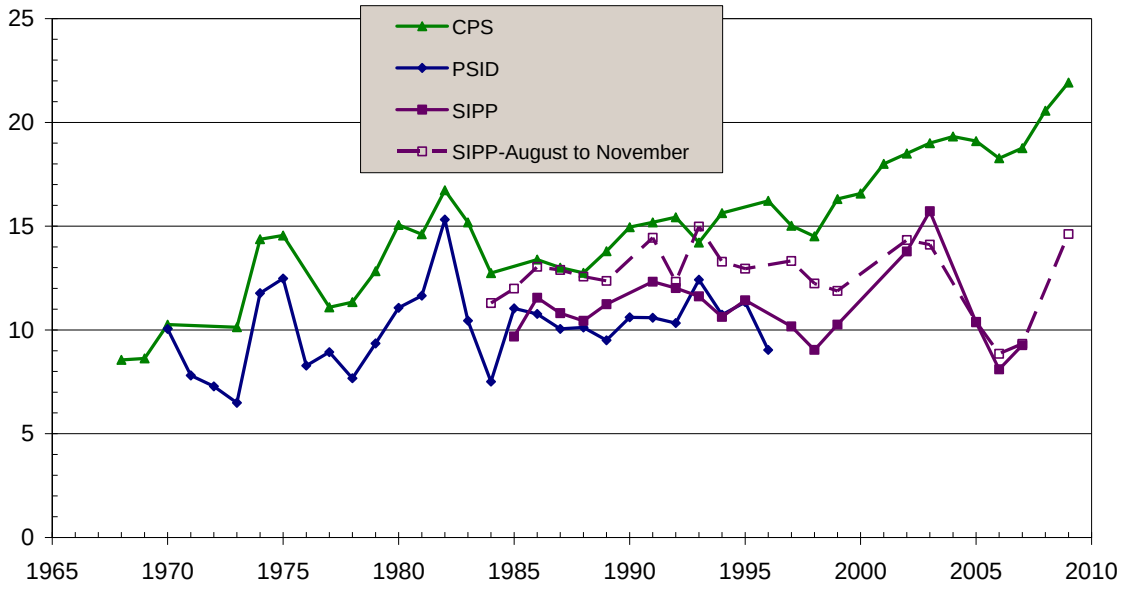

Figure 1b. Percent of Male Heads with Imputations for Wage and Salary Income

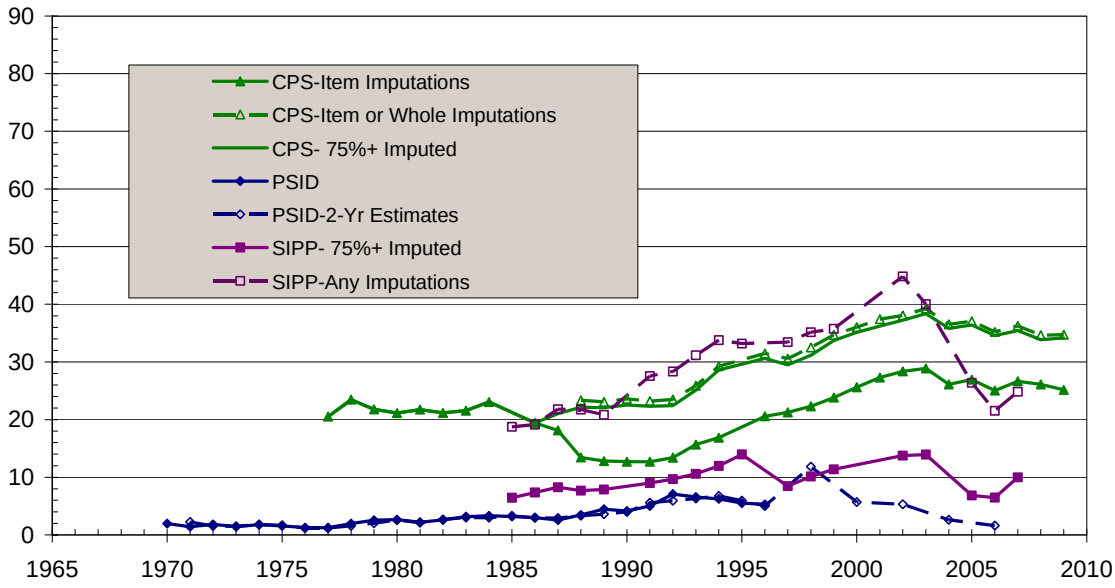

Figure 1c. Percent of Male Heads Experiencing One-Year Declines in Wage and Salary Income, No Imputations

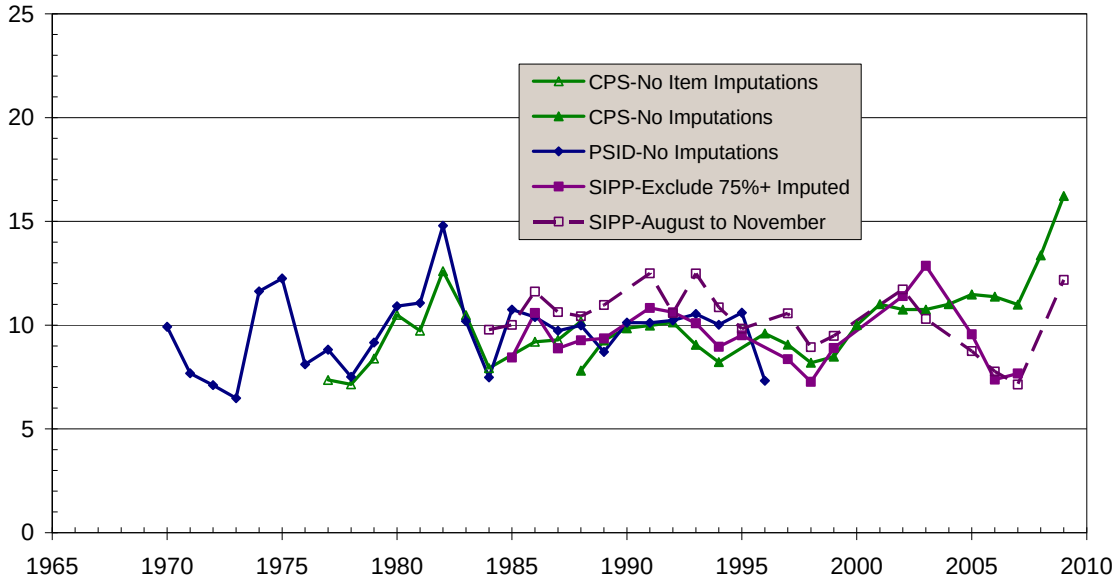

Figure 1d. Percent of Male Heads Experiencing Two-Year Declines in Wage and Salary Income

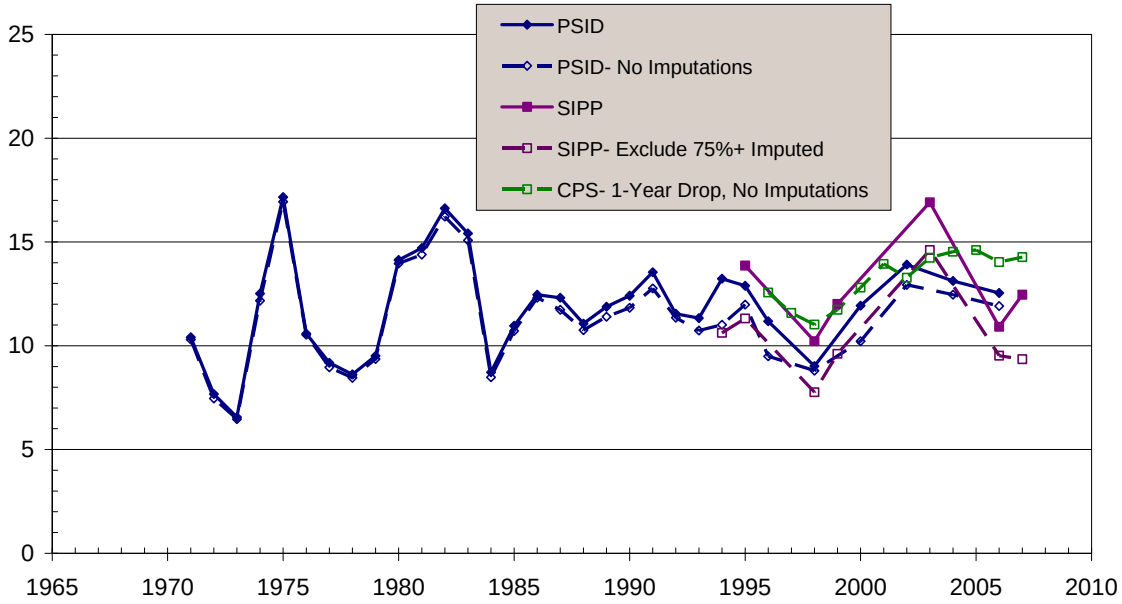

Figure 2a. Percent of Men Experiencing One-Year Declines in Earnings

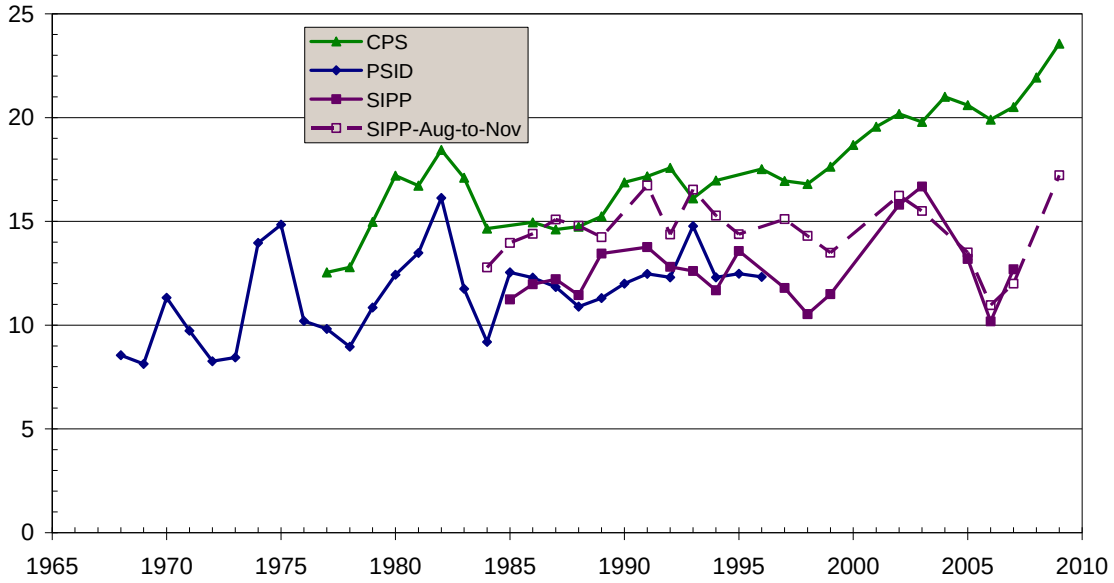

Figure 2b. Percent of Men with Imputations for Earnings

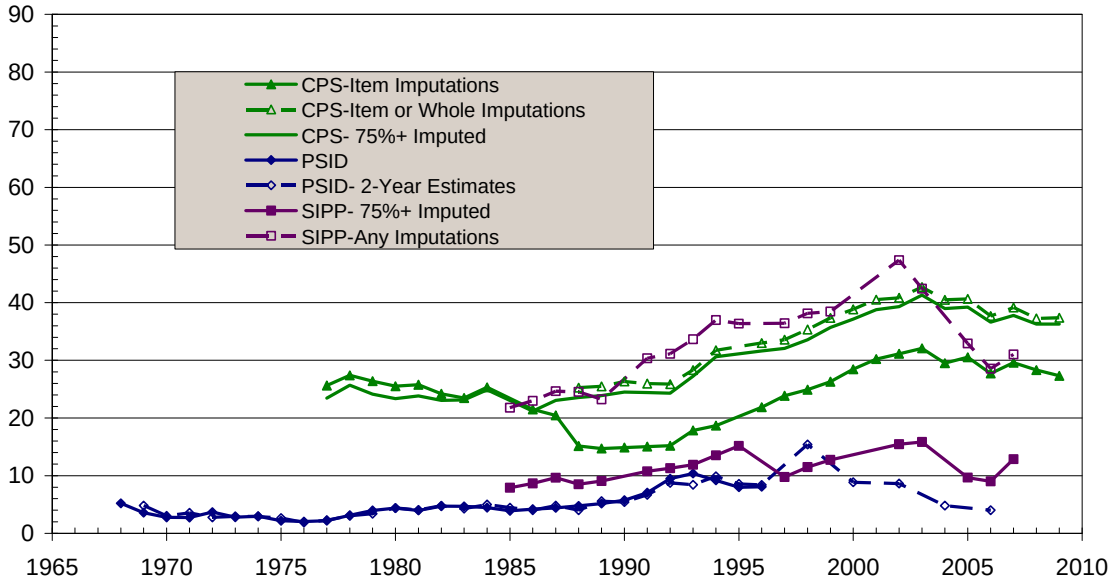

Figure 2c. Percent of Men Experiencing One-Year Declines in Earnings, No Imputations

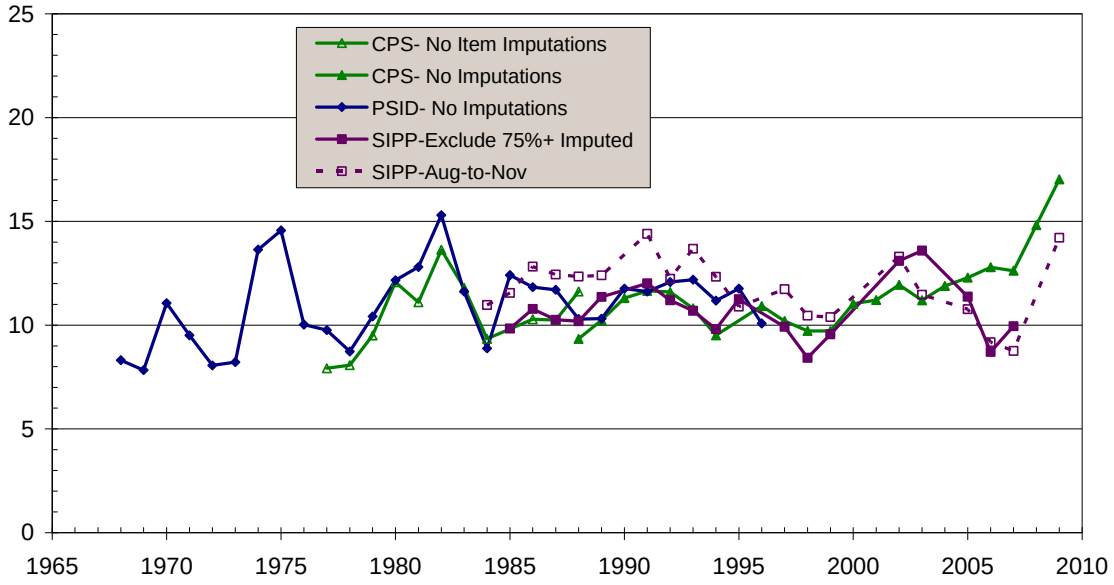

Figure 2d. Percent of Men Experiencing Two-Year Declines in Earnings

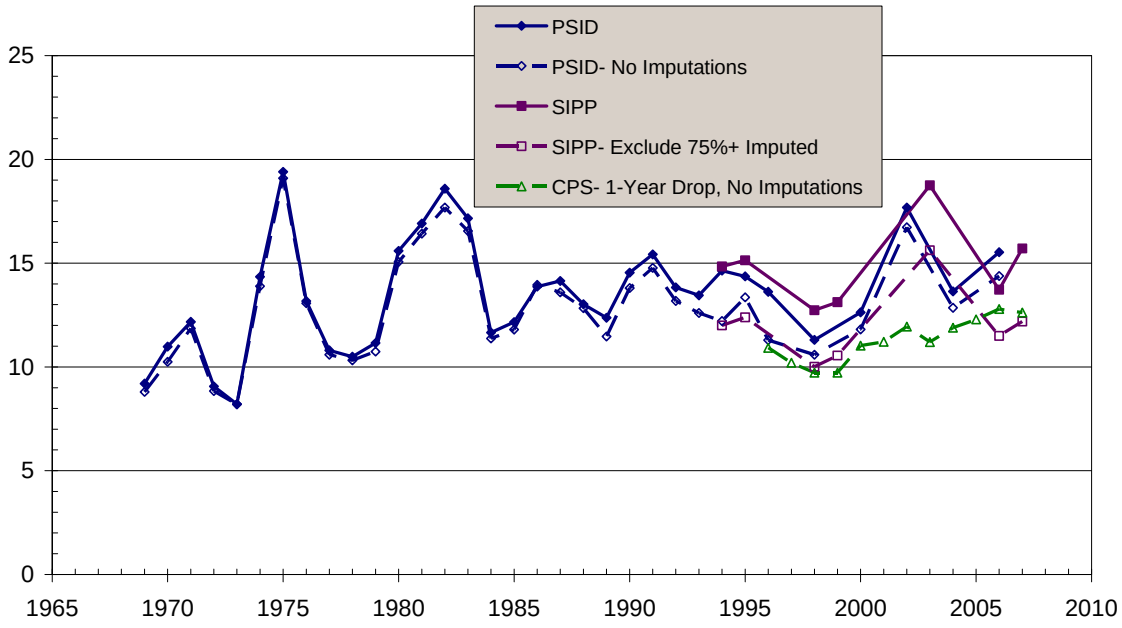

Figure 3a. Percent of Women Experiencing One-Year Declines in Earnings

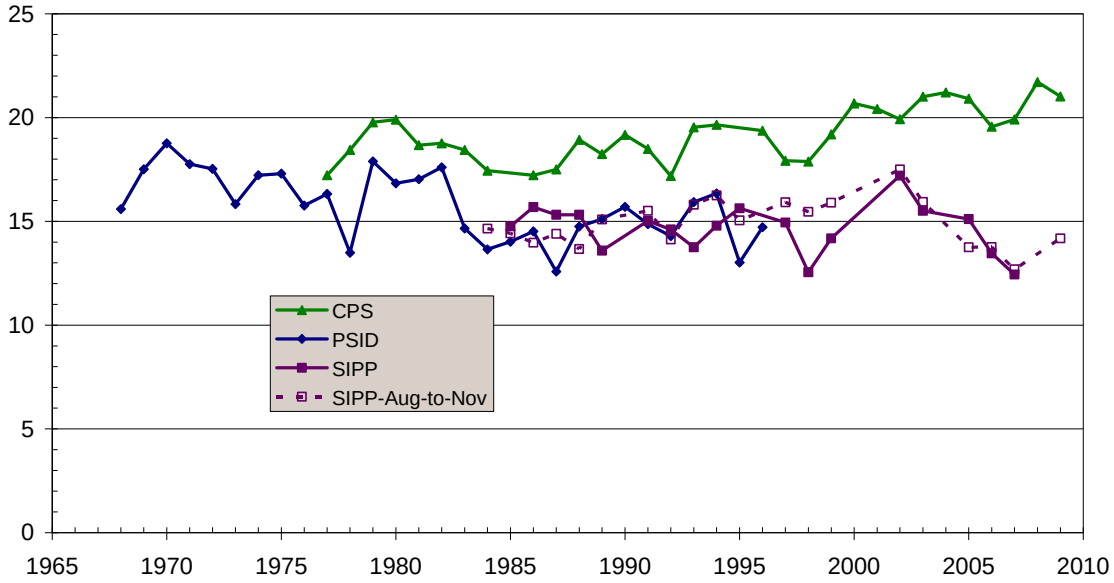

Figure 3b. Percent of Women with Imputations for Earnings

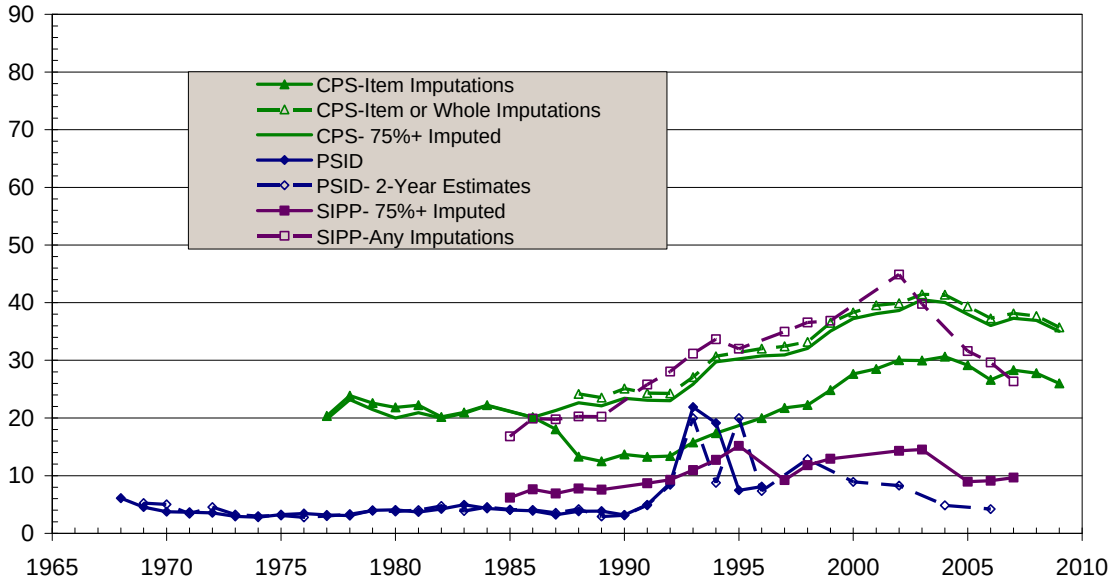

Figure 3c. Percent of Women Experiencing One-Year Declines in Earnings, No Imputations

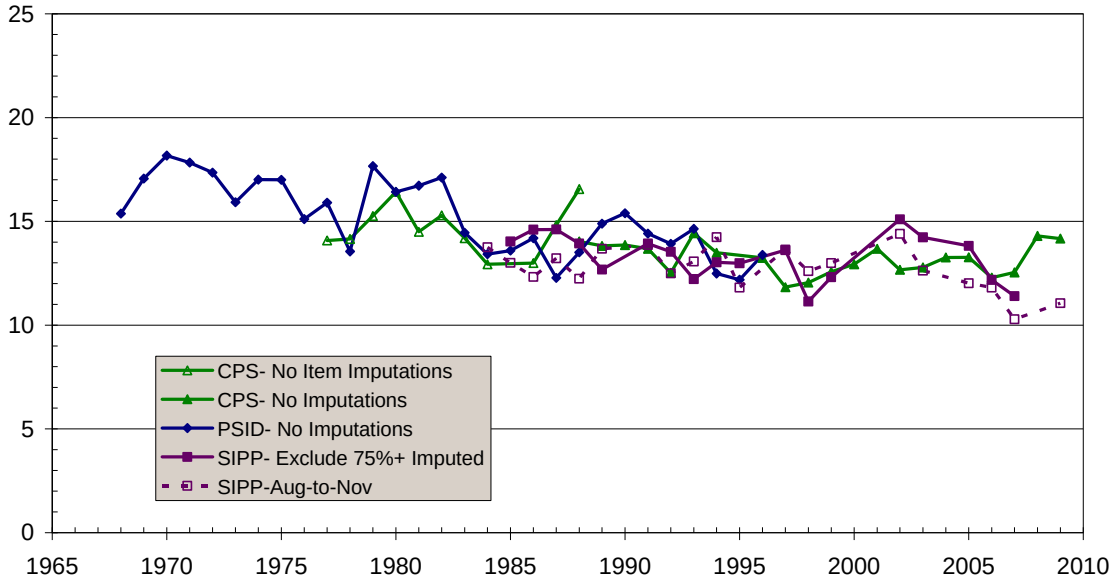

Figure 3d. Percent of Women Experiencing Two-Year Declines in Earnings

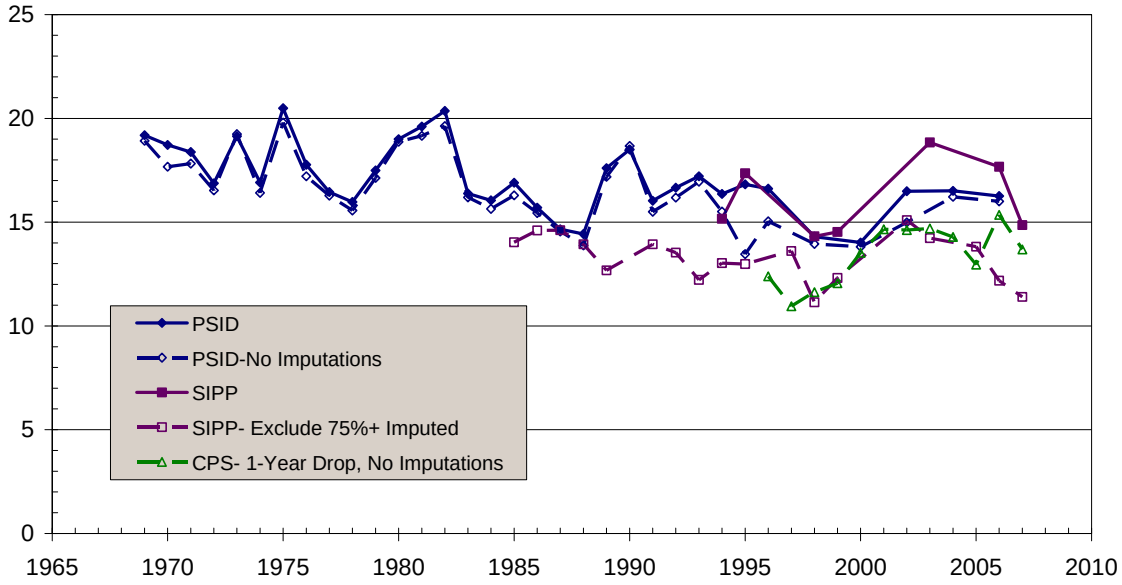

Figure 4a. Percent of Adults Experiencing One-Year Declines in Household Income

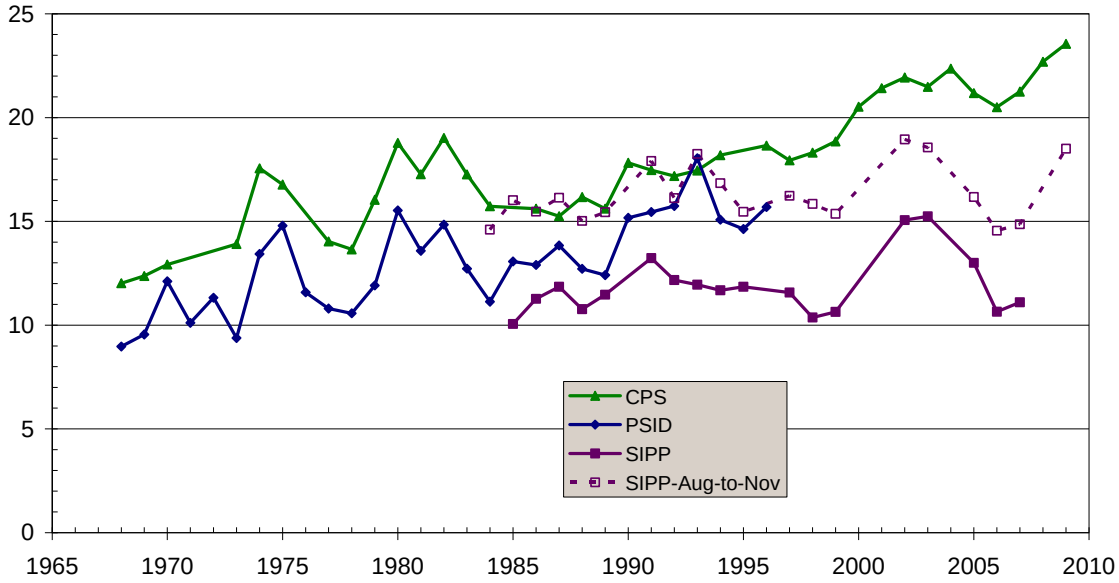

Figure 4b. Percent of Adults with Imputations for Household Income

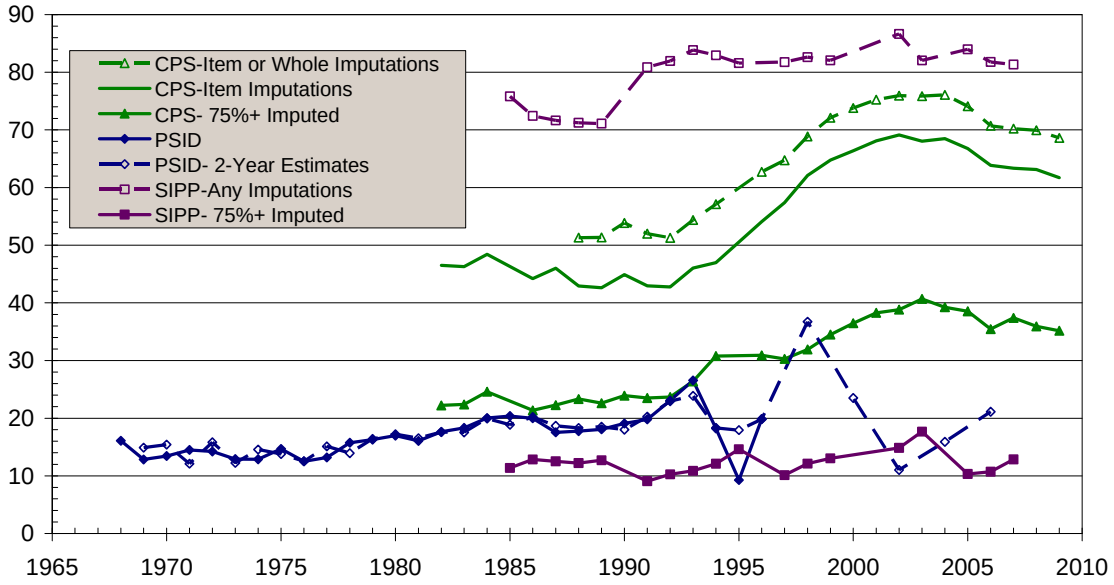

Figure 4c. Percent of Adults Experiencing One-Year Declines in Household Income, No Imputations

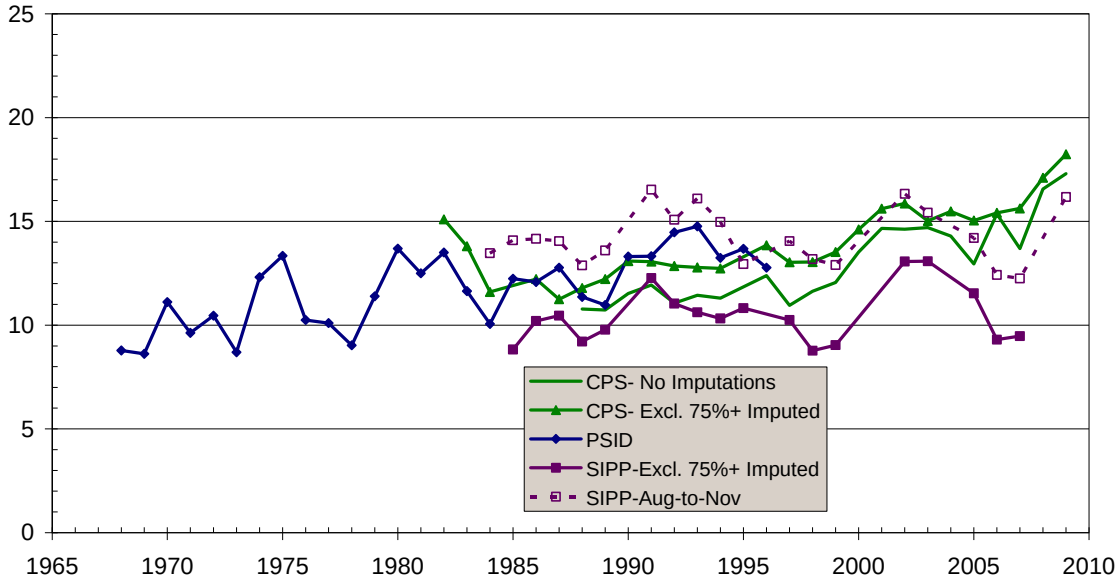

Figure 4d. Percent of Adults Experiencing Two-Year Declines in Household Income